style, and the introduction of numerous carefully executed maps and illustrations wherever opportunity is offered by the text.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Soldiers' Rations

IN your issue for June 28 (p. 158) Mr. H. Baden Pritchard states in his article on "Soldiers' Rations," "And yet, as we have said, with this apparently liberal feeding, our men do not receive so much actual nourishment, or nitrogenous matter, as the German soldier."

My calculations, based on Prof. Parkes' table of soldiers' rations, and Prof. Frankland's experiments on food and urea, give the following values of the several soldiers' rations :—

			Foot tons.
1.	English Military Prison		4,509
2.	English Soldier	(Home)	3,964
3.	Prussian ,,	(War)	3,812
4.	French ,,	(Crimea)	3,683
5.	French ,,	(Home)	3,580
6.	French ,,	(War)	3,538
7.	Austrian ,,	(Home)	3,242
8.	Prussian ,,	(on the march)	3,223
		Mean	3,694

As the average daily external work of a man is 353·75[1] foot tons, the efficiency of man regarded as a heat engine is 9·6 per cent. of the internal work.

An efficiency of 8·2 per cent. can be obtained by engines working at 40 lbs. pressure and steam cut off at half stroke ; so that man regarded as a machine, does not occupy a very high position. The explanation of this is, that man is not a *machine* ; he is a *machine maker*. The mechanism of a cat or beetle is vastly higher than that of man, and yet they are immeasurably his inferiors.

SAML. HAUGHTON

Trinity College, Dublin, July 7

Printing and Calico Printing

IN your article on the Caxton Exhibition last week, there is the remark that the beauty of execution in the specimens preserved to us of the work of the earliest known printers suggests a doubt whether the date of the actual invention must not be pushed back farther than the accepted one. But does that follow ? Is not the beauty of the first printing simply the beauty of the wood engraving of the time? an art which had attained an exquisite perfection before its application to movable type.

That there should be doubt and obscurity as to the date, name, and claim of the first inventor can surprise no one who will ask himself who, for instance, was the inventor of our present mode of calico printing by roller ; and, if he cannot answer, shall turn, as he has every right to do, to the current issue, ninth edition, of the "Encyclopædia Britannica" for the satisfaction of his doubt. He will find there, in that long, elaborate, and amply-illustrated article, not the bare mention of the name, even (unless I have strangely missed what I expressly looked for) of the otherwise remarkable man who conceived the idea, mechanically wrought it out for his own immediate purpose, and himself elaborated its application to the printing of calicoes—revolutionising that important branch of our industry—all well within the lifetime of men not half a century old among us ! The Rev. Isaac Taylor—turning a moment from his own researches into the Etruscan mystery—should be able to tell us in what precise year it was, after 1840, that his father, Isaac Taylor, the author of the "Natural History of Enthusiasm," and a long series of subsequent works, sufficient alone for a reputation of a high and lasting order—a teacher of teachers, the depth and extent of whose influence and the fulness of whose intellectual stature have not yet been adequately recognised—engraved on the roller illustrations for his new translation of Josephus, undertaken in connection with Dr. Traill. The death of his fellow-worker cut short that enterprise, but a portion of the work appeared ; and I myself, as a boy, was often in the little private workshop at Stanford Rives while this idea was struggling on the turning-lathe, through the patient genius of its author, for mechanical existence. In 1855 or 1856 I found him superintending its actual application to the printing of calicoes at Manchester. The discovery received the immediate and inevitable compliment of piracy, and brought to him and his loss instead of gain. But that within five-and-twenty years his very *name* should seem to have wholly dropped away from what was undoubtedly his own unaided invention, and one withal of so much national importance, and in an age of lime-light publicity like ours, is almost a curiosity of injustice, and throws, as I have said, a flood of light on a crowd of similar miscarriages in the indifferent past. As a hundred years hence this also may be beyond remedy, kindly assist me to arrest a moment the remorseless tooth of All-Father Time by the insertion of this contemporary note.

HENRY CECIL

Breigner, Bournemouth, July 9

Stamping out Noxious Insect Life

THE subject of insect and germ life in its relation to putrefaction and infectious disease is now assuming such importance from the investigations and demonstrations of Dr. Tyndall, Mr. Murray, and other scientific inquirers, that I think you may consider the following curious facts not unworthy of space in your journal.

I observe in a report of Dr. Tyndall's lecture on Germs, in NATURE, he refers particularly to the varying tenacity of life which germs under certain conditions exhibit, and which he refers to the period of incubation or stage of development up to the state of emergence as complete organisms, when they are readily destroyed. He says : "We now turn to another aspect of the question ; following the plain indications of the germ theory of putrefaction, we sterilise in five minutes the very infusions which, a moment ago, were described as resisting five hours' boiling. The germs are indurated and resistant, the adult organisms which spring from them are plastic and sensitive in the extreme. The gravest error ever committed by biological writers on this question consists in the confounding of the germ and its offspring. The active bacteria developed from those obstinate germs are destroyed at a temperature of 140° Fahr. Let us reflect upon these facts. For all known germs there exists a period of incubation, during which they prepare themselves for emergence as the finished organisms, which have been proved so sensitive to heat. If, during this period, and well within it, the infusion be boiled for the fraction of a minute, even before the boiling point is reached at all, the softened germs which are then approaching their phase of final development will be destroyed. Repeating the process of heating every ten or twelve hours, each successive heating will destroy the germs then softened, until after a sufficient number of heatings the last living germ will disappear. If properly followed out the method of sterilisation here described is infallible ; a temperature, moreover, far below the boiling-point suffices for sterilisation."

Now as the laws of nature apply to all magnitudes alike, whether it be a grain of sand or the planet Jupiter, to the various stages of incubation of the germs of bacteria or of noxious insect life, I think I may claim some credit for having *stumbled* upon, and for having applied on a practical and large scale, a system for eradicating insect life in animals based on this law of varying tenacity of life in germs and insects. More than two years ago I advocated this system, and in September last issued a circular, in which I stated that "a short time after clip-day I dipped, by immersion, the young lambs, and I repeated the same before harvest ; at the same time I made a long narrow pen alongside the stackyard fencing, into which I crammed all my old sheep as close together as possible. I then, with an ordinary watering-pan, watered them all over with diluted fluid ; the latter operation was completed in half an hour, and the cost in material was less than one halfpenny per head, the proportions in both cases being 1 to 100. Now for results ! I lately minutely examined the whole of my sheep, for the purpose of deciding if it was necessary to give them a final dressing before October, and I can now frankly, and without

---

[1] "Animal Mechanics," p. 62.

© 1877 Nature Publishing Group

hesitation, state that in the whole flock, old and young, I could not find a single living insect, or the germ of one."

In the month of March last, agreeably to my promise, I issued a faithful report of the results of this system of stamping out insect life. The sheep cleansed on this principle were absolutely clear of insect pests, whilst others not so treated were never free. I shall send with pleasure this report to any of your readers who may desire to have it.

I believe I may say there is exact analogy between this system for exterminating insect life in animals and that adopted by Dr. Tyndall to show that the earliest eggs or germs of bacteria are extremely obstinate to kill, whilst the more fully developed are destroyed without difficulty, clearly showing that more than one treatment is necessary for the complete destruction of germ life as well as for a higher form of insect life, and that the same law applies to both alike.

I am fully convinced of the possibility of stamping out noxious insects that affect sheep and other animals, and sincerely hope Mr. Murray's suggestion at the Society of Arts of united action to effect this purpose under the direction of science and experience may be acted upon with little delay. W. LITTLE

The Hall, Heckington, Lincolnshire

Complementary Colours

IN NATURE (vol. xvi. p. 150) you give a most interesting, though very brief, account of Prof. Rood's researches on colour, the result of which you sum up in these words :—

"The mixture with white is the same as if the colours were moved towards the violet end of the spectrum."

I know of Prof. Rood's results only from your abstract, but your summary is not a perfectly accurate account of the facts which you state immediately before, unless the expression "violet end" is to be used in a new sense, or, what would be better, replaced by the expression, "violet *pole*."

The following will be found a correct summary of Prof. Rood's results :—

Let the colours of the spectrum be arranged, not in a line but in a circle, and the gap between red and violet be filled up with purple. Each colour will then be opposite to its complementary colour. Greenish yellow and violet, which are mutually complementary, are the opposite poles, and the succession will be as follows :—

Greenish yellow.	
Yellow.	Yellowish green.
Orange.	Green.
Vermilion.	Cyanogen blue.
Purple.	Cobalt blue.
	Ultramarine.
Violet.	

The addition of violet to any one of the colours except those at the two poles will bring that colour nearer to the violet pole ; the same addition to either of the polar colours will leave them unchanged as to their position in the circle. *The addition of white will have the same effect.*

As regards the effect of the addition of violet, this is what we might expect. Violet added to violet will only make violet. Violet added in small quantities to greenish yellow, which is its complementary, will only make it whiter without changing the colour. Violet added to any other colour will bring it nearer to violet. We might select any pair of complementaries as poles, and obtain a parallel result. But what is new, and if confirmed, most important, is that white has the same effect as violet. I can suggest no explanation of this.

For the fact that every colour in the spectrum has its complementary, see Prof. Grassmann in the *Philosophical Magazine* of April, 1854. His paper appears to be less known than it deserves. The pairs of complementaries according to him are as follows :—

Red.	Bluish green.
Orange.	Azure.
Yellow.	Indigo.
Yellowish green.	Violet.
Green.	Purple.

Most authorities say that purple is not to be found in the spectrum, but Grassman says that under favourable conditions of light it may be. I think that in any observations on the subject where great accuracy is desired, the use of sunlight is to some extent misleading, and that the true white is that of an incandescent solid or liquid at a very high temperature—that is to say the electric light. The sun's light is such a light where it leaves the body of the sun, but part of its rays are absorbed in the sun's atmosphere, and the blue in greater proportion than the red and yellow ; and a further loss of blue rays takes place in the earth's atmosphere by scattering, forming the blue of the sky. The blue light of the sky is taken out of the white light of the sun. For these two reasons the sun's light at the surface of the earth is not truly white but yellowish.

I can scarcely doubt that when the spectrum of the electric light is carefully examined, it will be found to contain purple ; and also that some simple mathematical relation will be discovered between the wave-lengths of every colour and its complementary. JOSEPH JOHN MURPHY

Old Forge, Dunmurry, co. Antrim, June 24

Phyllotaxis

THE theory which regards the alternate arrangement of leaves as the normal mode receives some support from the arrangement of the inflorescence of opposite leaved plants. In *Lysimachia nemorum* the leaves are opposite, inflorescence indefinite, solitary, and axillary, but it will be observed that the flowers springing from the axils of opposite leaves are never both equally developed at the same time, one will be fully expanded while the other is yet in bud, or one will be found in seed and the other in flower ; it will be further observed that the oldest or most fully developed flower appears alternately on opposite sides of the stem ; if all the leaves on this plant were separated by internodes, the arrangement would be *tetrastichous*, but owing to the suppression of the internodes between the first and second and the third and fourth leaves, the arrangement becomes opposite. The oldest of the two opposite flowers of each pair of leaves will be found to spring from the axil of the first, third, and fifth leaf, and plants with this alternate disposition of flowers may sometimes be met with ; but usually a flower originates in the axil of each leaf, and then the youngest or latest flowers spring from the second and fourth leaves of the verticil ; these latter may be looked upon as originating from arrested branches. This view is supported by the fact that plants may be sometimes found which, in place of producing the late flowers in the axils of the third and fourth leaves, produce branches from these points instead. In *Caryophyllaceæ* the opposite sides of the cymose inflorescence never exhibit an equal amount of development at the same time, proving that one of the sides is older than the other, although, owing to non-development of an internode, it is at the same level. A similar arrangement occurs in *Labiatæ* plants, but owing to the crowded inflorescence, it is not so evident, but it is very marked where branches spring from opposite leaves ; one is generally two or three times as long as the other, and by tracing the arrangement of these long branches along the stem, the normal alternate arrangement may be determined. In *Scrophulariaceæ*, where both opposite and alternate leaves are met with, all the above-mentioned modifications may be seen. *Veronica chamædrys* has opposite leaves, and when the axillary racemes are opposite, one is invariably more developed than the other ; this can be best seen by examining the inflorescence in the young state, as the dissimilarity in size disappears in the pairs of old racemes owing to the younger of the two continuing to grow until it has acquired the size of the other ; sometimes this plant may be met with bearing in the axil of one of its opposite leaves a branch, and a raceme of flowers in the other, and in such instances the branches and flowers are produced on alternate sides ; in *V. officinalis* this is the usual arrangement. The suppression of the alternate nodes of an alternate-leaved plant with axillary inflorescence would produce the arrangement seen in *Lysimachia*, inasmuch as it would bring together flowers of different ages and in different stages of expansion ; but in this instance all the flowers would belong to the same generation or be the product of the same stem, whereas in *Lysimachia* the earliest developed of each pair of opposite flowers alone belong to the stem, while the later flower of each pair belong to another generation, and spring from a branch originating in the axil of the leaf opposite (owing to non-development of an internode) to the early flower ; the branch, however, is generally arrested, and the flower alone appears, although sometimes the branch is more or less developed.

G. E. MASSEE

© 1877 Nature Publishing Group